



# Interventions to prevent school exclusion

## Toolkit technical report

Hannah Gaffney, David P. Farrington and Howard White

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*This report is produced in collaboration with staff from the Campbell Collaboration Secretariat. It is a derivative product, which summarises information from Campbell systematic reviews, and other reviews, to support evidence-informed decision making’.*

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## **Abstract/Plain Language summary**

The objective of this technical report is to review the evidence on the effectiveness of school-based programmes to reduce school suspension or exclusion as an indirect prevention strategy for youth offending. In England, recent data shows that being excluded from school for a short period of time is more common and the rate of permanent exclusions is decreasing<sup>1</sup>. The report is mainly based on two systematic reviews: Mielke & Farrington (2021) and Valdebenito et al. (2018).

A range of different school-based interventions are used to reduce the use of school suspension and/or exclusion. Programmes typically target risk factors on both the individual and school levels and can include parents, teachers, school staff and the wider community. Interventions may thus seek to modify the behaviour of children, teachers or the 'whole school' (e.g., school rules and procedures).

Interventions to reduce the rate of school suspension or exclusion can be either targeted or universal programmes. Whole-school programmes, such as those designed to improve the school climate and/or change the disciplinary procedures, constitute universal prevention. Targeted programmes target students demonstrating problem behaviours in school who are at risk of exclusion.

There were significant differences in the dosage of included school-based interventions to reduce suspension. Mielke & Farrington (2021) state that targeted programmes involved between 5 and 165 hours of intervention activities. Universal programmes were implemented in schools for 1 – 4 years. Valdebenito et al. (2018) found that school-based interventions were mostly implemented for less than 12 weeks in just over one-third of cases, and for more than 24 weeks for a similar proportion.

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<sup>1</sup> <https://explore-education-statistics.service.gov.uk/find-statistics/permanent-and-fixed-period-exclusions-in-england>

Our headline estimate for violence and crime outcomes is based on the impact of exclusion programmes on arrests. The observed effect size from the review by Mielke and Farrington (2021) of 0.013 corresponds to an approximate reduction of 2% in arrests. The evidence rating is 3.

The estimated reduction in exclusion varied from 4% for Mielke & Farrington (2021) to 35% for Valdebenito et al. (2018). Valdebenito et al. (2018) also found that the reduction fell to 19% after 12 months or more. The evidence rating for the impact on exclusion outcomes is 4 for the Valdebenito et al. (2018) review and, due to the indirect nature of the estimate on crime and violence outcomes, the evidence rating is 2 for these outcomes.

It is possible that the effect of programmes was found to be greater in the Valdebenito et al. (2018) review because the evaluations covered a longer time period (1980-2015, compared with 2008-2019 for Mielke & Farrington, 2021), and their review included a larger number of evaluations (37, compared with 12 results for exclusion in Mielke & Farrington, 2021). Also, whereas Valdebenito et al. (2018) included all types of exclusion, Mielke and Farrington (2021) only included out-of-school suspension, thereby excluding in-school suspension and expulsion.

The most effective types of interventions for suspension/exclusion were: (1) violence reduction (2) mentoring/monitoring (3) counselling, mental health; and (4) enhancement of academic skills. Important moderators were predominantly male schools and interventions targeting students rather than teachers. Neither age nor ethnicity made a difference to programme impact.

In England and Wales, school exclusion programmes generally seek to identify children with problem behaviour and to intervene to prevent exclusion. Such approaches need strong commitment from school leadership, in-school support for both teachers and children and help to connect with local services. Successful engagement of families, though difficult, is often an important part of a successful programme. The benefit-to-cost ratio of a programme which placed social workers in schools in England was reported to be 2.5.

## **Objective and approach**

The objective of this technical report is to review the evidence on the effectiveness of school-based programmes to reduce school suspension or exclusion, as an indirect prevention strategy for youth violence.

This technical report is mainly based on two systematic reviews: Mielke & Farrington (2021) and Valdebenito et al. (2018). The following inclusion and exclusion criteria were used to inform the selection of systematic reviews.

### *Inclusion criteria*

To be included in this report a systematic review must:

- Review school-based intervention programmes that aim to reduce suspension and/or exclusion from school.
- Review interventions implemented with children and adolescents that were 'school-age', i.e., between 4 and 18 years old.
- Report quantitative effects on school suspension or exclusion.
- Be published recently.

### *Exclusion criteria*

Reviews were excluded for the following reasons:

- The review did not include school suspension or exclusion outcomes.
- The review did not use systematic methods to search, identify and evaluate evaluations.
- The review did not carry out a meta-analysis.

## Outcomes

The primary outcomes of interest in this technical report are school suspension and school exclusion. The most recent data for England<sup>2</sup> show that, for the academic year 2018-2019, the rate of permanent exclusions from school was 0.1%<sup>3</sup> estimated as the number of exclusions as a proportion of the overall school population. In total, 7,894 students were permanently excluded from school in England during 2018-2019. The prevalence of fixed period exclusions (i.e., suspensions/ temporary exclusion) was 5.36% in that year, and a total of 438,265 students were excluded from school for a fixed period.

School exclusion disproportionately affects ethnic minorities. Valdebenito et al. (2018) report data from the Department for Education for England for Travellers of Irish Heritage, Black Caribbean and Gypsy/Roman ethnic groups. Black Caribbean pupils were twice as likely to be subject to fixed-period exclusion and four times more likely to be permanently excluded. The racial bias in exclusion remains even when controlling for other factors in multivariate analysis.

Previous studies have demonstrated that being suspended or excluded from school is associated with a range of undesirable outcomes. For example, Rosenbaum (2020) – discussed later -- found that school suspension predicted an increased likelihood of involvement with the criminal justice system. Therefore, school suspension or exclusion is an important risk factor for youth offending and interventions designed to reduce the rate of suspending or excluding students from school are likely to reduce the rate of offending.

Mielke and Farrington (2021) included outcomes of school suspension and arrest. Suspension from school was measured using official school district measures and could have been in-school or out-of-school suspension. In-school suspension refers to incidences where a student is removed from normal classes but is supervised separately at the school. Out-of-school

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<sup>2</sup> <https://explore-education-statistics.service.gov.uk/find-statistics/permanent-and-fixed-period-exclusions-in-england>

<sup>3</sup> The source of this information presents these figures in this way. We assume that they mean that 0.1% of all school children were permanently excluded in that year.

suspension refers to incidences where a student is sent home and not allowed to attend school for a designated period of time. Mielke and Farrington (2021) did not include studies that included only in-school suspension or studies that included only permanent exclusion from school. Arrest was measured as the proportion of students who were arrested by the police within a specific time frame or the average number of arrests per young person in the sample. This information was obtained from official police or criminal justice records.

Valdebenito et al. (2018) reported 37 effects of school-based intervention programmes on school exclusion, and included in-school exclusion, out-of-school exclusion, and expulsion (permanent exclusion). The review also included studies that reported a general measure of suspension/exclusion, or in other words, no operational definition of suspension or exclusion was provided.

### **Description of interventions**

A range of different school-based interventions are used to reduce the use of school suspension and/or exclusion, and programmes typically target risk factors on both the individual and school level and can also include parents, teachers, school staff and the wider community (Valdebenito et al., 2018). Interventions may thus seek to modify the behaviour of children, teachers or the 'whole school' (e.g., school rules and procedures).

Mielke and Farrington (2021) reviewed 14 intervention programmes implemented in school settings that were designed to reduce suspension from school for behavioural problems. Such interventions aim to improve students' social and emotional skills which in turn, "may help students manage their behaviour, succeed in school, and avoid involvement with the criminal justice system" (Mielke & Farrington, 2021, p. 2). Programmes were implemented across different school levels, with five including only elementary school students (i.e., aged 5 – 11 years old), one including only middle school students (i.e., aged 11 – 14 years old), and four

including only high school students (i.e., 14 – 18 years old; Mielke<sup>4</sup> & Farrington, 2021). Four studies evaluated programmes that were implemented across different school levels.

These school-based interventions are designed as alternative behavioural management to suspension or exclusion. Given the negative impact that exclusion from school can have on long-term outcomes throughout the lifespan, such as offending and criminal justice involvement, these interventions provide schools with alternative ways to provide behavioural support, protect staff and students, and also reduce further antisocial behaviour.

### *Targeted or Universal*

Interventions to reduce the rate of school suspension or exclusion can be either targeted or universal programmes (Mielke & Farrington, 2021). Whole-school programmes, such as those designed to improve the school climate and/or change the disciplinary procedures, constitute universal prevention. These interventions can be described as using a systems-based approach and aim to improve the school as a system in order to change outcomes for students.

School-based interventions to reduce suspension or exclusion can also be targeted. These programmes target students who are demonstrating problem behaviours in school (e.g., classroom disruption, insubordination, bullying, violence towards teachers or other students) and offer alternative ways to manage and change their problem behaviours.

### *Intervention components*

The school-based intervention programmes to reduce suspension reviewed by Mielke and Farrington (2021) and Valdebenito et al. (2018) involved a range of different activities. Targeted programmes incorporated elements of social-emotional learning, cognitive behavioural therapy, or a combination of both (e.g., Rochester Resilience Project, Positive Action programme, 'Becoming a Man' programme; Wraparound case management). Cognitive behavioural therapeutic techniques are used to help students regulate their behaviour and provide youth with appropriate coping strategies and mechanisms. Social-

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<sup>4</sup> Studies were categorised according to the US education system classification. The typical age range of students in elementary, middle and high school are provided for transparency, but are based on generalisations.



emotional learning interventions can target a range of different skills, such as self-awareness, self-regulation, social awareness, relationship and communication skills, and decision-making. Such programmes target the root causes of problem behaviours that may result in a young person being suspended or excluded from school.

Programmes that targeted specific youth who were demonstrating behavioural problems in school also included components of academic support or tutoring (e.g., Match Tutoring, Early College High School programme). These programmes involved students with low academic achievement receiving personal tutoring. For example, the 'Match Tutoring' programme paired male middle school students with college graduates (without formal teaching training) for one-on-one tutoring in maths to help the student improve performance in this subject.

Whole-school interventions focused on restorative practices (e.g., 'SaferSanerSchools' programme) or packaged school-based programmes like the 'Schoolwide Positive Behavioural Interventions and Supports' (SWPBIS; Mielke & Farrington, 2021). These programmes target conflict resolution skills and provide practical skills to manage anger, become more assertive, or resolve conflicts (Valdebenito et al., 2018). Typically, these interventions involve a range of components and activities such as one-to-one instruction, modelling, role-play exercises, feedback and reinforcement (Valdebenito et al., 2018). Whole-school approaches may include clear displays of the school's policy or other anti-bullying material around the school, and frequent mention of the policy in school assemblies or other settings.

Restorative practices, such as responsive circles and restorative conferences, are used in universal programmes to manage conflict between students. For example, the 'SaferSanerSchools' programme focused on communication skills to create positive environments, encouraging students to take responsibility for their actions and using restorative principles after disruption or conflict has occurred.

### *Implementing personnel*

Valdebenito et al. (2018) reported that school-based intervention programmes to reduce school suspension and/or exclusion were implemented by a range of individuals. For example,

programmes were implemented by teachers, social workers, psychologists or counsellors, police/probation officers, or trained community agents.

Whole-school or classroom-based programmes to reduce school suspension or exclusion are typically implemented by teachers in their respective classrooms, or by school principals across the whole school. For example, the Positive Action programme involves a social-emotional learning curriculum that is implemented in the classroom by trained teachers (Mielke & Farrington, 2021). Classroom level intervention components include teachers' ability to manage student behaviour, set rules, maintain attendance and reinforce positive behaviour (Valdebenito et al., 2018).

#### *Duration and Scale*

Mielke and Farrington (2021) reported that there were significant differences in the dosage of included school-based interventions to reduce suspension. Targeted programmes involved between 5 and 165 hours of intervention activities. Universal programmes were implemented in schools for 1 – 4 years (Mielke & Farrington, 2021).

Valdebenito et al. (2018) found that school-based interventions were mostly implemented for less than 12 weeks ( $n = 14$ ; 37.8%) or for more than 24 weeks ( $n = 14$ ; 37.8%). Four evaluations (10.8%) were implemented for between 13 and 24 weeks. The mean number of weeks was 20.4 and the mean number of hours of intervention per week was 1.78 hours.

#### *Example of interventions*

A well-known evidence-based example of a school-based behavioural intervention programme is the School-wide Positive Behavioural Interventions and Supports (SWPBIS; Mielke & Farrington, 2021; Valdebenito et al., 2018).

The SWPBIS programme aims to reduce the use of school suspension and change the overall school climate and disciplinary procedures. The programme involves focusing on creating a positive school climate and setting “universal positive expectations” for student behaviour (e.g., improving school climate and reducing problem behaviours; Valdebenito et al., 2018). There are then suitable interventions implemented with students who do not meet these

behavioural standards (Mielke & Farrington, 2021). The programme is based on a multi-level approach and both whole-school and targeted intervention activities are included. SWPBIS offers group-based intervention for students with problem behaviours and also personalised tailored intervention programmes for individual students. Valdebenito et al. (2018, p. 32) outline the core elements of the SWPBIS programme as:

- Building a school culture for both social and academic attainment
- Early prevention of problem behaviours
- Teaching social skills to all students
- Using behaviour support practices
- Actively using data for decision-making

### **Theory of change/presumed causal mechanisms**

The presumed causal mechanism in school-based interventions depends on who is the target of the intervention. The majority of interventions are targeted at the child, including elements of whole-school approaches which make clear a 'no tolerance' policy. These interventions are intended to improve school behaviour, and hence reduce school suspension and/or exclusion, which will in turn lead to reductions in antisocial behaviour, offending and violence. Classroom-based approaches may also improve teacher management of disruptive pupils so that the teacher is less likely to resort to exclusion. And, finally, the school exclusion trial in England made schools responsible for Alternative Provision, thus incentivising them to keep children in their regular class and implement programmes which achieve this aim.

Given the extensive literature suggesting that young people who are excluded from school face further adverse life experiences, by reducing the incidence of school exclusion, these undesirable outcomes can be prevented.

## Evidence base

### *Descriptive overview*

Mielke and Farrington (2021) reviewed 14 evaluations, published in 2008-2019, of school-based programmes to reduce school suspension ( $n = 12$ ) and/or arrest of youth ( $n = 6$ ) using randomised controlled trials. All of the studies were evaluated in the USA, with the exception of Osbuth et al. (2017) who report the evaluation of a programme implemented in London. Of the twelve evaluations that reported effects on suspension from school, the majority of programmes were designed to reduce suspension ( $n = 8$ ) and most were considered universal intervention programmes ( $n = 8$ ). Six of these evaluations were described as having large sample sizes and six were described as having small sample sizes (Mielke et al., 2021, p. 16). No further information about the male-female ratio or ethnic background of participants included in evaluations is provided.

Valdebenito et al. (2018) included 37 evaluations in their meta-analysis of school-based interventions to reduce school suspension/exclusion. Evaluations were published between 1980 and 2015 and the mean date of publication was 2003. 51% of evaluations were published and 49% were unpublished. Most of the evaluations were conducted in the USA, with just three from the UK, and all were randomised controlled trials. The average sample size was 1,168 participants and the average age of participants was 12.9 years old. Across studies with ethnicity data, 54.1% of participants on average were Black, 24.6% were White, and 20.2% were Latino. The mean percentage of students eligible for free school meals was 66.2%. No information about the gender of participants was reported. Most of the interventions were implemented at the student level (73%) in comparison to the school level (27%). Programmes were implemented by external facilitators and school facilitators (32.4%) or school facilitators only (32.4%).

### *Assessment of the evidence rating*

We have confidence that, at the time of writing, the reviews by Valdebenito et al. (2018) and Mielke and Farrington (2021) represent the best available evidence on the effectiveness of school exclusion prevention programmes. Our decision rule for determining the evidence rating is summarised in the technical guide.

Two independent coders used a modified version of the AMSTAR2 critical appraisal tool was used to appraise the reviews by Valdebenito et al. (2018) and Mielke and Farrington (2021). According to this tool, the review by Valdebenito et al. (2018) was rated 'high' and the review by Mielke et al. (2021) was rated 'low'. The results of this assessment are summarised in Annex 3.

Mielke and Farrington (2021) published their report in a peer-reviewed journal and Valdebenito et al. (2018) conducted a Campbell systematic review, which are known to be of very high methodological standards and was also independently published in a peer-reviewed journal (Valdebenito et al., 2019).

Both reviews adequately specified the research questions and the inclusion/exclusion criteria. The inclusion criteria included components relating to the population, intervention, comparison group and outcome of interest. Specifically, Mielke and Farrington (2021) state that evaluations must have evaluated an intervention based in a school setting, used an experimental design with at least 100 students and report impact on official records of arrest or suspension. Valdebenito et al. (2018) included school-based or school-supported interventions to reduce rates of suspension from school for children in mainstream schools aged between 4 and 18 years old. Furthermore, Valdebenito et al. (2018) specify that evaluations must have viewed the intervention programmes as an alternative to school exclusion.

Valdebenito et al. (2018) registered a protocol in the Campbell Collaboration Library of Systematic Reviews and specify throughout the review where there were any deviations from the protocol. Mielke and Farrington (2021) did not include any information about a review protocol.

Both reviews only included randomised controlled trials or, as Mielke and Farrington (2021) describe, experimental designs in which students (or schools) were randomly assigned to either a treatment or control group. Valdebenito et al. (2018) provide a detailed rationale for why other methodological designs were excluded. For example, this review excluded quasi-

experimental studies that did not report sufficient baseline measures or did not use appropriate matching procedures.

Both reviews reported a comprehensive literature search strategy including a number of different databases, designated keywords and search strategies. Neither of the reviews restricted inclusion criteria to only peer-reviewed publications or only reports in English, but Mielke and Farrington (2021) do not report searching any databases of unpublished literature or any specific databases that would return non-English results. Valdebenito et al. (2018) specify that they searched databases for non-English reports (e.g., SciELO, an electronic database for scientific publications from developing countries, published in Spanish/Portuguese).

Mielke and Farrington (2021) do not provide any information about the coding of studies or whether studies were coded by more than one person. Valdebenito et al. (2018) state that coding was carried out by two of the authors and inter-rater reliability was assessed. Valdebenito et al. (2018) also included detailed information about studies that were excluded from the review and the reasons why they were excluded.

Valdebenito et al. (2018) evaluated risk of bias using the EPOC risk of bias tool, as suggested by the Campbell Collaboration, and conducted a series of analyses to evaluate the impact of possible risk of bias on outcomes. Mielke and Farrington (2021) did not conduct any risk of bias analyses, beyond publication bias analysis.

Mielke and Farrington (2021) state that no funding was received for their review and Valdebenito et al. (2018) state that the review was funded by the Nuffield Foundation, but no conflict of interest is suspected.

Each of the reviews conducted a meta-analysis and reported detailed information on the synthesis and estimation of weighted effect sizes and adequately reported the heterogeneity between primary effects. Each of the meta-analyses reported separate weighted effect sizes for independent outcomes and assessed multiple moderators as possible explanations for heterogeneity between primary effect sizes.

Mielke and Farrington (2021) report a direct estimate of the effect on arrests based on 6 evaluations of school exclusion prevention programmes. There was a relatively low amount of heterogeneity between primary studies ( $I^2 = 14\%$ ) but a small number of evaluations and rated 'low' as per AMSTAR tool, such that the evidence rating is 3.

Mielke and Farrington (2021) also report a direct estimate of the effect on school suspensions based on 12 evaluations. However, there was high heterogeneity between primary evaluations ( $I^2 = 83\%$ ) and the 'low' rating as per the AMSTAR tool, such that the evidence rating is 3. However, due to the indirect nature of the estimate on crime and violence, the evidence rating is 2.

Valdebenito et al. (2018) report a direct estimate of the effect on school exclusion based on 38 evaluations with high heterogeneity between primary evaluations ( $I^2 = 88\%$ ). Thus, the evidence rating is 4 for school exclusion outcomes. However, due to the indirect nature of the estimate on crime and violence outcomes, the evidence rating is marked down to 2.

## **Impact**

### *Summary impact measure*

Overall, Valdebenito et al. (2018) found that school-based interventions were effective in reducing school suspensions and school exclusion. Table 1 outlines the summary mean effect sizes for the reviews that inform this technical report (Mielke & Farrington, 2021; Valdebenito et al., 2018).

Mielke and Farrington (2021) suggest that included programmes reduced suspension from school and arrests of youth, but the weighted mean effect size was not statistically significant. Valdebenito et al. (2018) found that school-based intervention programmes significantly reduced school exclusion in the experimental group compared to the control group. However, Valdebenito et al. (2018) found that the mean effect decreased for outcomes that were measured 12 or more months following the intervention. From 12 studies that reported data on outcomes of school exclusion after 12 or more months, the mean effect size was  $d = 0.15$  and was not statistically significant (95% CI -0.06, 0.35).

Table 1

*Mean effect sizes for school suspension, exclusion and arrest.*

Review	ES ( <i>d</i> and OR)	CI (ES)	<i>p</i>	% reduction	Evidence rating for school exclusion outcomes	Evidence rating for crime and violence outcomes
Mielke & Farrington (2021); suspensions	<i>d</i> = 0.033 OR = 1.06	-0.036, 0.102	.215	4%	3	2
Mielke & Farrington (2021); arrests	<i>d</i> = 0.013 OR = 1.02	-0.003, 0.029	.093	2%	n.a.	3
Valdebenito et al. (2018); exclusion	<i>d</i> = 0.30 OR = 1.72	0.20, 0.41	< .001	35%	4	2
Valdebenito et al. (2018); exclusion after 12+ months	<i>d</i> =0.15 OR = 1.31	-0.06, 0.35	0.171	19%	4	2

*Note:* ES = the weighted mean effect size; CI = 95% confidence intervals for the mean ES; *p* = the statistical significance of the mean ES; OR = odds ratio; *d* = Cohen’s *d* under a random effects model of meta-analysis; positive values of *d* indicate desirable decreases in outcome (as do OR values greater than 1.0); n.a. = not applicable.

If we assume equal numbers in the experimental and control conditions (e.g., *N* = 100 participants in each condition) and a suspension rate of 25% in the control condition (for consistency with other technical reports), the odds ratio for Mielke and Farrington (2021) of 1.06 for school suspension corresponds to a relative decrease of approximately 4%. This estimate is not greatly affected by different assumptions. With similar assumptions, the



reduction in Valdebenito et al. (2018) is 35%, but just over half that (19%) at the 12 month or more follow up. These transformations are explained in further detail in Annex 1.

The prevalence of exclusion varies with many factors, including the demographics of the school and the time period covered. As mentioned, data from England and Wales shows that 5.36% of all school children were excluded from school for a fixed period in one year.

In an evaluation, the actual prevalence of exclusion in a no-treatment control group (i.e., a school that does nothing to prevent exclusions) may be less than 25%. Of course, in the UK, current policies in all schools are generally designed with the aim of reducing exclusions, given the undesirable outcomes for the child.

If we assume that the prevalence of exclusion is 10%, the mean effect sizes from the Valdebenito et al. (2018) review translate to a 39% relative reduction in exclusions immediately and a 22% relative reduction in exclusions 12 months later. Similarly, the mean effect size on suspensions from the Mielke and Farrington (2021) review translates to a 5% relative reduction in suspensions (see Annex 1).

Both reviews reported significant heterogeneity between primary evaluations, although to varying degrees (Mielke & Farrington, 2021:  $Q = 63.61$ ,  $df = 11$ ; Valdebenito et al., 2018:  $Q = 301.3$ ,  $df = 36$ ). Both reviews explored different moderators to explain possible reasons for heterogeneity between primary evaluations.

Valdebenito et al. (2018) reported the weighted mean effect sizes for different types of school exclusion. This review found that school-based interventions were most effective in reducing expulsions ( $d = 0.53$ , 95% CI 0.07, 0.98,  $p = 0.024$ ,  $n = 4$ ). Interventions were similarly effective in reducing in-school exclusion ( $d = 0.35$ , 95% CI 0.11, 0.58,  $p = .004$ ,  $n = 6$ ) and 'general' exclusion ( $d = 0.32$ , 95% CI 0.21, 0.43,  $p < .001$ ,  $n = 27$ ). Valdebenito et al. (2018) report a small non-significant effect of interventions on out-of-school exclusion ( $d = 0.02$ , 95% CI -0.16, 0.19,  $p = .848$ ).

### *Suspension/Exclusion and Later Offending*

School suspension and exclusion predict later offending. For example, Rosenbaum (2020) carried out a very large scale, long term follow-up study of a nationally representative sample of U.S. youth in the National Longitudinal Study of Adolescent and Adult Health (Add Health). She noted that one-third of U.S. students were suspended during their school career. She followed up a sample of 480 students who were suspended for the first time in 1995-96 and a matched sample of 1,193 non-suspended youth. By 2008, 28.2% of the total sample had been arrested at least once, and suspended youth were 38% more likely to be arrested than non-suspended youth (relative risk = 1.38, 95% confidence interval = 1.18 to 1.60). Based on her figures, 35.1% of suspended youth were arrested, compared with 25.44% of non-suspended youth. Therefore, if all suspended youth had not been suspended, the arrest rate might have decreased by 27.5%.

Therefore, we can expect that reductions in suspension would be followed by reductions in offending and violence. To the extent that both suspension and offending are behavioural manifestations of the same underlying theoretical construct (e.g., an antisocial personality), then, if this is decreased by the school programme, we might expect that offending would be similarly decreased; in other words, that a decrease of 19% in the prevalence of suspension might be followed by a decrease of 19% in the prevalence of offending.

However, if this is not true, and decreases in suspension cause decreases in offending, we might expect that the consequent decrease in offending would be less than the observed decrease in suspension. Based on Rosenbaum (2020), if all suspended youth were not suspended, we might estimate that offending could decrease by about 2%. However, this estimate is quite speculative and would vary with different assumptions. More longitudinal follow-ups of school suspension programmes are needed to study later effects on offending.

### *Moderators and mediators*

Mielke and Farrington (2021) conducted a number of moderator analyses on data for school suspensions, but there were not enough ( $n = 6$ ) effect sizes for arrests to carry out further

analyses. A number of moderators were included, and the results suggest that the following comparisons were statistically significant<sup>5</sup>:

- Universal programmes ( $d = 0.058$ , 95% CI 0.025, 0.091,  $p = .002$ ) were more effective than targeted programmes ( $d = -0.047$ ; 95% CI -0.106, 0.012,  $p = .095$ ) in reducing school suspension.
- Interventions were significantly more effective with older students in high school ( $d = 0.137$ , 95% CI 0.055, 0.22,  $p = .003$ ) than interventions implemented with younger students in elementary schools ( $d = 0.021$ , 95% CI -0.03, 0.072,  $p = .251$ ).
- Programmes designed to reduce suspension were significantly effective in reducing school suspension ( $d = 0.097$ , 95% CI 0.059, 0.135,  $p < .001$ ) but programmes not specifically designed to reduce school suspension were not ( $d = -0.050$ , 95% CI -0.093, -0.006,  $p = .031$ ).
- Evaluations categorised as 'high' on the implementation rating were associated with reductions in suspension ( $d = 0.110$ , 95% CI 0.071, 0.149,  $p < .001$ ) but evaluations in the 'low' category were not ( $d = -0.047$ , 95% CI -0.103, 0.009,  $p = 0.84$ ).
- Evaluations categorised as having a small sample were significantly associated with a reduction in school suspension ( $d = 0.237$ , 95% CI 0.144, 0.331,  $p < .001$ ) but evaluations with large sample sizes were not ( $d = 0.012$ , 95% CI -0.018, 0.042,  $p = .261$ ).

Valdebenito et al. (2018) compared groups of included evaluations based on the participants' demographic characteristics, behavioural problems, the theoretical basis of interventions and the quality of the intervention. The most significant moderator was whether or not the evaluation was run by the developer of an intervention ( $b = -0.36$ , SE = .14,  $p < .05$ ). This result suggests that evaluations that were run by a team independent of the development of the programme were associated with smaller effect sizes than evaluations conducted by the team who also developed the intervention.

The remaining (i.e., not statistically significant) results are summarised as follows:

- Programmes implemented in predominantly male schools were associated with greater effectiveness ( $d = 0.41$ , 95% CI 0.10, 0.72,  $p < 0.05$ ) compared to programmes evaluated in mixed-gender schools ( $d = 0.17$ , 95% CI 0.02, 0.32,  $p < .05$ ) but the difference was not statistically significant.
- There was no evidence that intervention programmes were more or less effective in relation to participants' age.

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<sup>5</sup> In moderator analyses with meta-analytical data, the statistical significance is impacted by the number of studies included in the analyses. It is also important to discuss non-statistically significant results, which are also summarised.

- There was no evidence to suggest that ethnicity of participants was related to the effectiveness of a programme.
- The most effective types of interventions were: (1) violence reduction ( $d = 0.48$ , 95% CI -0.33, 1.3,  $p > .05$ ,  $n = 3$ ); (2) mentoring/monitoring ( $d = 0.47$ , 95% CI 0.02, 0.93,  $p < .05$ ,  $n = 5$ ); (3) counselling, mental health focus ( $d = 0.46$ , 95% CI 0.23, 0.68,  $p < .001$ ,  $n = 3$ ); and (4) enhancement of academic skills ( $d = 0.43$ , 95% CI 0.25, 0.61,  $p < .001$ ,  $n = 2$ ). Interventions described as ‘violence reduction’ were specifically aimed to increase self-control and reduce violence (Valdebenito et al., 2018, p. 62).
- Interventions that targeted change at the student level were associated with greater effectiveness ( $d = 0.33$ , 95% CI 0.19, 0.48) compared to interventions that targeted change at the school level ( $d = 0.25$ , 95% CI 0.04, 0.45). The difference between the two groups of studies was not statistically significant.

There were a number of moderators for which the subgroup comparisons had a lack of statistical power due to too few primary evaluations to adequately detect a significant difference. More evaluations and further moderator analyses are needed.

## Implementation

In July 2014, the Department for Education<sup>6</sup> evaluated the ‘School Exclusion Trial’ in which schools were made responsible for Alternative Provision to address the needs of and provide support for permanently excluded students and those at risk of permanent exclusion. The evaluation included schools from 11 local authorities and assessed issues surrounding the implementation of the trial with views from students, schools, local authorities and alternative provision providers.

This study was not a randomized trial, and so not included in either of the included reviews. Hence impact findings are reported here along with observations on implementation. These results are summarised as follows:

- Schools reported that there were fewer children on average who had been excluded from trial schools in comparison to control schools, and more trial schools retained responsibility for excluded children. Trial schools “were taking an increased moral and practical responsibility for pupils at risk of exclusion” (DfE, 2014, p. 10). There was also

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[https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment\\_data/file/331796/RB364\\_-\\_School\\_Exclusion\\_Trial\\_Final\\_Report\\_Brief.pdf](https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/331796/RB364_-_School_Exclusion_Trial_Final_Report_Brief.pdf)

a decrease in the number of children categorised as being at risk for exclusion in trial schools.

- Local authorities reported that, overall, trial schools were taking increased responsibility for students at risk of exclusion and so were working to place young people in appropriate alternative provisions.
- Alternative provision included a range of options, such as placing students in other schools or referral to specialist support services (e.g., Child and Adolescent Mental Health Services). Other effective alternative provisions included work placements or time spent in further education colleges.
- Whilst teachers reported that policies on exclusion in their respective schools did not change greatly as a result of participation in the trial, schools did change in relation to use of early intervention and behavioural support, use of alternative provision and collaboration with other schools.
- Throughout the trial all schools adopted inclusion/learning support units and the use of 'time out' sessions decreased. Learning support units, inclusion coordinators, and revised school timetables were thought to be effective in reducing exclusions, improving attendance, behaviour and attainment in both trial and comparison schools.
- Teachers and local authorities reported that weaknesses of alternative provision related to the process (e.g., the time, logistics, issues around timetabling and cost), the provision (e.g., quality control and monitoring of alternative provisions) and a lack of students and/or caregiver engagement.

Another evaluation, commissioned by the Paul Hamlyn Foundation, reports qualitative findings from three different projects with rather different approaches to reducing school exclusion (Smith et al., 2013). The approaches are (i) a staged approach to managing interventions for children deemed at risk of exclusion; (ii) engaging with families to improve parenting practices and hence child behaviour, and (iii) therapeutic support to children at risk of exclusion. The common element of these programmes is that they are preventive interventions, intending to address problem behaviour to prevent exclusion. A common finding is that having staff to manage these cases, for example liaison with local services, takes a burden off teachers so that they can focus on teaching. Success factors are identified for each project (see Annex 2). In each case, the commitment of the school leadership to the programme is identified as being important. It can be difficult to engage families at first, but is an important part of effective delivery – especially of course for programme components which work with families.

### *The Timpson review*

In 2019, a government commissioned review of school exclusion in England and Wales found that, whilst exclusion from school should remain as a tool that headteachers can use to ensure a safe school environment, there is much that can be done to improve outcomes for students who are excluded (Timpson, 2019). In particular, Timpson highlights the need to support children and particularly those who are vulnerable, such as children with special education needs or children who are already disadvantaged in life. The review also suggested that children from BAME backgrounds, in particular Black and mixed-race children, were more likely to be excluded from school. Timpson (2019) identified several recommendations for improved practice, specifically on issues of leadership in schools, equipping schools to better handle problem behaviours, providing incentives for schools to use exclusion more appropriately and ensuring strong safeguards to protect children from being inappropriately excluded from school.

### **Cost analysis**

Neither review included information on the cost of school-based interventions to reduce school suspension/exclusion. However, two anti-bullying studies from the United Kingdom, which have reductions in school exclusion as a planned outcome, report cost data.

In the Learning Together study of a whole-school anti-bullying intervention with socio-emotional learning for all pupils and restorative justice sessions to deal with bullying episodes, expenditures on anti-bullying programmes were £108 per pupil per year in control schools and £166 per pupil in treatment schools (at 2019 prices), indicating an additional cost of £58 per pupil.

Bagley and Pritchard (1998) present analysis of a programme for two schools with a full time Project Social Worker, a full-time project teacher in a primary school and a half time teacher in a secondary school. Programme benefits included reduced truancy and exclusion as well as disruptive behaviour and bullying. The total cost of the programme at the time was £177,000

over the three years of the study which is equivalent to £411 per pupil per year at 2019 prices. The authors reported that the intervention had a benefit-to-cost ratio of 2.5.

### **Findings from UK/Ireland**

Mielke and Farrington (2021) included one evaluation of a programme implemented in the UK (Osboth et al., 2017) and found that the programme did reduce the number of arrests, although not statistically significantly so ( $d = 0.174$ ,  $SE = 0.127$ ,  $p = .086$ ,  $n = 738$ ). Moreover, no significant impact on school suspension was found ( $d = -0.203$ ,  $SE = 0.214$ ,  $p = .172$ ,  $n = 714$ ). In fact, the latter effect suggests that, following the intervention, the rate of suspension from school was lower in the control group than in the intervention group.

Osboth et al. (2017) evaluated the implementation of the 'Engage in Education' programme in a London secondary school. The programme targeted students who were at risk of school exclusion and aimed to improve their social skills. Intervention components included anger management, self-calming and de-escalating strategies, assertive communication and helping students to identify behaviour alternatives in a variety of contexts. The programme was implemented by trained external professionals over 12 group sessions and 12 one-on-one sessions. Osboth et al. (2017) note that some schools reported "organisational and logistical difficulties" in implementing the programme (Mielke & Farrington, 2021, p. 13) and overall, the programme was not well attended.

An earlier evaluation reported findings from the 'Meeting Need and Challenging Crime in Partnership with Schools' project which was conducted in collaboration with two local education authorities in the North East of England (Vulliamy and Webb, 2003). Five school-based, home-school support workers worked in six secondary schools and one age 11-14 middle school.

The work of the support workers involved supporting younger siblings and families of target pupils, providing crisis-management support when in-school incidents took place, supporting teachers in managing pupils with emotional and behavioural difficulties, identifying alternative curriculum provision such as work experience placements and further education

college courses, helping to establish whole-school policies on behaviour; and building links with Social Services, Health and other outside agencies which reduced the workload on teachers. These support workers worked with 208 pupils at risk of exclusion over the three years of the project.

The evaluation found that a number of permanent exclusions was cut during the project period, with an estimated 26 permanent exclusions averted by the project. Whilst the number of fixed-term exclusions continued to rise it did so at a slower rate.

### **What do we need to know? What don't we know?**

Despite the scale of the problem, there are few studies of school exclusion in the United Kingdom. It would be useful to establish the extent to which the roll out of mental health services in schools, and other developments such as the Liaison and Diversion Service, have affected school exclusions. This would need to be an ex post facto study exploiting differences in service roll out, design and intensity, complemented by a qualitative research component.

Research on innovative, effective and low-cost strategies to reduce school exclusions is needed. For example, in an intervention reviewed by Valdebenito et al. (2018) the focus was on an empathy-based philosophy. The intervention focussed on encouraging teachers to adopt empathetic attitudes on student discipline and it had low-cost and long-term effectiveness in reducing school exclusions (Okonofua et al., 2016). There is also ongoing research into the application of restorative justice practices to reduce school exclusions. This is potentially a promising area of research that warrants more research and evidence-based practice.

The Learning Together study of a whole school approach to bullying planned to analyse school exclusion but was unable to obtain the necessary data from the schools. The study team considered undertaking the analysis once the administrative data on school exclusions was available, but will no longer do so. Since this would be a useful addition to the limited UK evidence base, this possibility should be explored.





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## Annex 1: Effect size calculation

This annex shows the calculation based on the results and assumptions given in the text. We assume 200 youth, evenly divided between treatment and comparison groups. That means there are 100 youth in the control group and 100 youth in the treatment group. Assuming that 25% of youth in the control group are excluded, the mean effect sizes for Valdebenito et al. (2019) can be easily transformed to a percentage reduction in reoffending. We assume a prevalence of 25% in the control group for comparability with other technical reports.

If the odds ratio for school exclusion is 1.72, then using the table below, we can estimate the value of X. The odds ratio is estimated as:  $A \cdot D / B \cdot C$ , where A is the number of students not excluded in the treatment group, B is the number of students excluded in the treatment group, C is the number of students not excluded in the control group, and D is the number of students excluded in the control group. Therefore, the value of X is 16.23 in the case of Valdebenito et al. (2018).

	Not		
	excluded	Excluded	Total
Treatment	100-x	x	100
Control	75	25	100

Therefore, the relative reduction in school exclusion is  $[(25 - 16.23)/25] \cdot 100 = 35.08\%$ . In relation to exclusion after 12 months, the value of X is 20.28 and the relative reduction is 18.88%.

The prevalence of being excluded is likely to vary between studies and can be influenced greatly by factors such as the definition of suspension/exclusion, the time period covered, school disciplinary practices, the availability of alternate provisions and national-level educational policy. If we were to adjust our assumption that 25% of the control group are excluded, the relative reduction in the intervention group is not greatly affected.

For example, if we assume that 10% of the control group are excluded, the 2x2 table would be as follows and the value of X would be 6.068 for the Valdebenito et al. (2018) review. Therefore, the relative reduction is 39.32% (i.e.,  $(10 - 6.068)/10 \times 100$ ).

	Not		
	excluded	Excluded	Total
Treatment	100-x	X	100
Control	90	10	100

Similarly, if we assume that 40% of the control group are excluded, the value of X would be 27.93 for the Valdebenito et al. (2018) review, and the relative reduction would be 30.18%. Despite the dramatic difference in the assumed prevalence of being excluded, the relative reduction does not vary in a similar fashion. Table 2 shows this further.

Table 2

*Variation of the relative reduction in school exclusions/suspensions depending on various estimates*

	Valdebenito et al. (2018) OR = 1.72	Mielke & Farrington (2021) OR = 1.06
Assumed prevalence	Relative reduction	
10%	39.32%	5.12%
25%	35.08%	4.31%
40%	30.18%	3.48%

## Annex 2: Overview of projects evaluated in Smith et al. (2015)

	Description	Success factors
Care Guidance Support Stages (CGSS), Mounts Bay secondary school in Penzance, Cornwall	The CGSS is an approach to managing behaviour in schools which aims to identify pupils with behavioural issues as early as possible, and offer targeted support to address these issues. It is designed to facilitate positive relationships and communication between pupils, schools and parents. The support is managed and delivered via the PHF-funded Intervention Coordinator, who designs and trains Learning Support Managers (LSM) to deliver Skill Workshops, coordinates case study care plans, manages the externally funded Shifting Horizons Forest School learning programme, directly supports pupils and facilitates their access to other services. As such, the CGSS is a whole school intervention that tailors support depending on the nature of the individual pupil's situation.	Key to the success of the CGSS is (1) a rigorous administration process, and (2) the role of the PHF-funded Intervention Coordinator. Appropriate balance between a punitive and supportive approach. Strong leadership and communications around the system to help embed it into the school routine and culture and ensure that the CGSS did not get 'lost' amongst other priorities.
SWIFT, Family Groups Feltham and Hanworth, London Borough of Hounslow	Family Group is a highly targeted intervention working with children and parents in school-based, multi-family therapy sessions. Therapeutic sessions are run by the School and Family Works (SFW) and a school-based partner (usually the deputy head or someone in a pastoral role) and work with the whole family to address problems rooted in family relationships and dynamics. Sessions take place weekly, in school, for half a day. Families continue to attend for as long as necessary, rather than for a fixed number of sessions	Effective engagement of parents. They must recognise that they are not there to 'fix a problem with the child', but to change their own parenting approach. Empathetic, non-judgemental support of the therapists Group format, which enables parents to support each other, building trust so that they can share their problems and mutually develop solutions. Strong backing from schools is vital.
Learning 2 Learn, Teignmouth Community School, Devon	Learning 2 Learn provides therapeutic support in primary schools to children who have been, or are identified as being at risk of being, excluded. L2L is underpinned by the THRIVE approach, which draws on current thinking in neuroscience, attachment theory, child development, and research into the role of creativity and play in developing emotional resilience. The project has a base at Teignmouth Community School, which is called the Nest. Much of the support is delivered there.	The pre-existence of the Learning Community which has a history of collaborating with the primary schools in the area. Having support from the highest levels in the lead school. Project is complementary to, but not part of, the schools. Ensure sustainability of its impact, through THRIVE training of school staff who can implement the underpinning principles in their interactions with children

### Annex 3: AMSTAR Quality Rating

Modified AMSTAR item		Scoring guide	School exclusion	
			Mielke & Farrington 2021	Valdebenito 2018
1	Did the research questions and inclusion criteria for the review include the components of the PICOS?	To score 'Yes' appraisers should be confident that the 5 elements of PICO are described somewhere in the report	Yes	Yes
2	Did the review authors use a comprehensive literature search strategy?	At least two bibliographic databases should be searched (partial yes) plus at least one of website searches or snowballing (yes).	Yes	Yes
3	Did the review authors perform study selection in duplicate?	Score yes if double screening or single screening with independent check on at least 5-10%	No	Yes
4	Did the review authors perform data extraction in duplicate?	Score yes if double coding	No	Yes
5	Did the review authors describe the included studies in adequate detail?	Score yes if a tabular or narrative summary of included studies is provided.	Yes	Yes
6	Did the review authors use a satisfactory technique for assessing the risk of bias (RoB) in individual studies that were included in the review?	Score yes if there is any discussion of any source of bias such as attrition, and including publication bias.	Partial Yes	Yes
7	Did the review authors provide a satisfactory explanation for, and discussion of, any heterogeneity observed in the results of the review?	Yes if the authors report heterogeneity statistic. Partial yes if there is some discussion of heterogeneity.	Yes	Yes
8	Did the review authors report any potential sources of conflict of interest, including any funding they received for conducting the review? Overall	Yes if authors report funding and mention any conflict of interest	Yes  Low	Yes  High





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